into a shape something like the pointed half of the bowl of a spoon. Another statement appears to me of questionable accuracy. The author notices the earth pillars on the southern slopes of the Eggishorn, describing them correctly, but saying of them, "Les pyramides des fées, aussi appelées 'blocs perchés.'" Surely this is an unwonted extension of the latter term.

The pamphlet, in short, is rather disappointing. It is beautifully printed on quarto pages with large margins, and is illustrated with three photogravures of glacier scenery, which would be improved by the omission of the human figures, for these by contrast look like negroes in mourning; but it tells us little that is new, and is a "popular" article rather than a scientific memoir.

T. G. Bonney.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* Nature. *No notice is taken of anonymous communications.*]

Panmixia.

Private communications which I have received from naturalists interested in this controversy, and from Mr. Romanes himself, have thrown light on the apparently irreconcilable difference of the views which have been expressed.

I think it desirable that an explanation should be afforded to the readers of Nature.

When Mr. Romanes contends that cessation of selection leads to a dwindling in the size of a useless organ, he now tells me that he assumes that the mean size of the part in all born (what we may call the birth-mean) was smaller than the mean size of that part in those individuals surviving under selection. Hence the withdrawal of selection substitutes in the adult survivors the lower birth-mean for the former higher selection-mean.

Mr. Romanes had not specifically stated that he made this assumption.

On the other hand, I had—for the purpose of estimating purely and solely the result of panmixia and cessation of selection—assumed that birth-mean and selection-mean were identical, in which case the withdrawal of selection would, of course, not alter the mean.

To assume that birth-mean is smaller than selection-mean in a given case seems to me to be introducing causes other than panmixia or cessation of selection.

It is evident that cases are *possible* in which the mean given by selection is identical with the birth-mean—others in which it is *smaller* than the birth-mean, and others in which it is *larger*. Special causes of a complex character determine whether the ratio is one or the other. If we are to consider the effects of cessation of selection alone, apart from other causes, it seems to me that we must not introduce causes which affect the ratio of birth-mean and selection-mean; we must eliminate them altogether by assuming the ratio to be one of equality. Hence my conclusion that panmixia or cessation of selection alone cannot produce the dwindling of an organ.

If, however, we admit the assumption that the selection-mean is larger than the birth-mean, Mr. Romanes has my full concurrence in stating that cessation of selection leads to dwindling, and I am of course aware that, given that assumption, Weismann and Galton are of the same mind.

The point of interest therefore shifts. The question is, whether we are justified in assuming that in organisms generally in a state of nature the mean size of an organ or part in the selected survivors is larger than in all born, or, to put it fully, larger than would have been the mean size of the part in all born supposing that they had all reached maturity.

I do not think that we have data which warrant this assumption. It is, I think, certain that some cases must sometimes occur in which this is the case, and others in which the selection-mean-size is *smaller* than the birth-mean-size. It is not improbable that in well-established species there is identity of the two means. This is, however, a question which ought to be settled by observation—*not* of domesticated races, but, if possible, of wild forms.

It seems to me that this assumption is precisely what Mr. Darwin considered, and refused to make, so that he avoided attributing dwindling of parts to the cessation of selection. He says ("Origin," 6th ed., p. 401): "If it could be proved that every part of the organization tends to vary in a greater degree towards diminution than augmentation of size, then we should be able to understand how an organ which has become useless would be rendered, independently of the effects of disuse, rudimentary, and would at last be wholly suppressed." Mr. Darwin says, "If it could be proved." This is really the whole point. If the greater size of selection-mean than of birth-mean could have been proved, Mr. Darwin was ready to formulate the doctrine of dwindling by cessation of selection. But, apparently, it could not be proved then. It has not been proved yet. I do not think it at all impossible that it may be proved. The facts are as yet not recorded.

E. Ray Lankester.

May 10.

Bertrand's Idiocyclophanous Spar-prism.

It is a good thing that Prof. Silvanus Thompson has brought the above prism to the notice of the Physical Society (see Nature, vol. xli. p. 574); it is certainly remarkable that M. E. Bertrand himself has never thought fit to publish any description of his interesting invention. Perhaps it may be worth while to mention a fairly simple method of constructing the prism (which may easily have occurred to others besides myself, and) which has the advantage of requiring only two artificially-worked surfaces, and hence of interfering as little as possible with the natural rhombohedral crystal of Iceland spar.

Four plane, polished faces are required for the prism, which is, in fact, a four-sided parallelopipedon, having two opposite sides parallel to the optic axis, while the two others make an angle of 45° with it.

Now, since in Iceland spar the faces of the natural rhombohedron make angles of very approximately 45° (strictly, 45° 24′) with the optic axis, two of these faces can be utilized for the last-mentioned pair of prism-sides.

Take, then, a cleavage-rhomb of spar, about 1 cm. in thickness, and having edges about 4 cm. in length (Fig. 1); observing that both the face A B C D and the opposite one, A′ B′ C′ D′, are flat and free from blemishes (such a crystal is easily found, even in these spar-famine days). Grind away the solid angle A′ down to about the level shown by the dotted lines, working the face thus obtained so that it makes an angle of 45° with the natural face A B C D. Cut away the opposite solid angle C in a similar way, so as to make another plane, parallel to the first. Polish the two cut surfaces, and the prism is complete in all essential particulars.

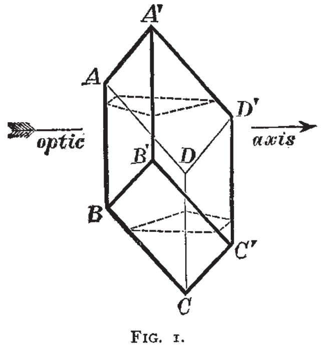

Fig. 1.

Thus, if a beam of common white light is allowed to fall normally on one of the worked surfaces, A, Fig. 2 (which is a section of the prism), it will be (1) totally reflected at the natural face B (corresponding to A B C D in Fig. 1); (2) pass on through the crystal parallel to the optic axis; (3) undergo another total reflexion at the opposite natural face C; and (4) finally emerge through the second worked plane D. An eye placed close to D will then observe the well-known pair of ring-systems side by side, one set complementary to the other.

A very convenient source of illumination seems to be a lamp-

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flame within a globular opal shade, placed at such a distance that the three images of it produced by the action of the prism (the centre image formed, of course, by the superposition of two, similarly polarized) just touch each other. Two of these images are then filled (like a lantern-disk) with the complementary ring-systems; and by a very slight motion of the crystal the rings pass from a given disk to the adjacent one, becoming complementary in so doing. (It is hardly necessary to explain, for no doubt Prof. Thompson did so fully, that the whole prism is precisely equivalent to a train of two double-image prisms with refracting angles of 45°, having between them a plate of spar with surfaces at right angles to the optic axis; a "Huyghens apparatus," in fact, with an interposed spar-plate instead of the usual selenite film.)

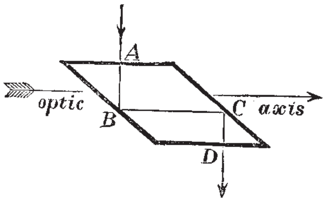

Fig. 2.

I may add that I have found it convenient, in order to demonstrate the principle of the prism, to divide it into halves; or, more strictly, to cut a piece of spar so as to form one half of the prism only, as shown in Fig. 3. Then, if common light from the lamp-shade (as described above) is allowed to enter the face A, and a tourmaline is held in the path of the rays emergent from C, ring-systems are seen just as when a double-image prism is used as a polarizer and a plate of spar held in front of it. Also, if plane-polarized light is allowed to enter C, and the eye is held close to A, ring-systems are seen side by side; that portion of the spar through which the rays pass after total reflection at B acting, of course, exactly as a double-image analyzer. In fact, the prism may, in this position, be used alone as a "Savart's polariscope" to detect traces of polarization in sky-light, &c. But for this application, the prism would possess, in the eyes of the true votary of science, the inestimable value of being of no practical utility whatever.

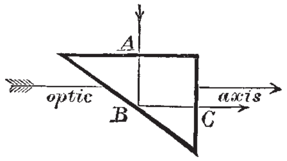

Fig. 3.

Queen's College, Oxford. H. G. Madan.

Coral Reefs, Fossil and Recent.

Dr. von Lendenfeld's account of the dolomites of the Italian Tyrol, in his letter on "Coral Reefs, Fossil and Recent," is a very valuable contribution to this interesting question; but I think he can hardly have fallen in with the new edition of Darwin's "Coral Reefs," or he would not have asserted that in the discussion "the structure of our Triassic limestone mountains has been left out of account." In the appendix (p. 332) I wrote:—"If those geologists are right who consider the Schlern dolomites as being to a great extent due to reef-building corals, we have, in the Triassic deposits of the Italian Tyrol, reefs thick enough to satisfy the most exacting requirements." I could not venture upon a more positive statement, because I knew controversy on this question was not ended, and I had not myself, though fairly familiar with the "Dolomites," discovered evidence which appeared to me conclusive (though I incline to the above opinion myself), and because I considered that the view advanced several years since by Richthofen required some modification—indeed, as to one detail, if I understand him rightly, I should differ from Dr. von Lendenfeld.

I am confirmed in my idea that he has not read this book, because I find that one of his chief arguments—that against the indefinite lateral extension of a coral reef on a talus of its own building—appears to correspond with one advanced by myself on p. 327, differing only in the addition of an arithmetical example; one of which, indeed, I did work out, but afterwards suppressed as needless, the truth of the statement being obvious when it was once pointed out. T. G. Bonney.

Bison and Aurochs.

In regard to Prof. Newton's letter in your issue of the 8th, I beg to state that in restricting the name aurochs to the European bison, I have merely followed the general custom of English zoologists.

Citing a few authorities, I may first make the following extract from a paper by Prof. W. B. Dawkins, published in the Quart. Journ. Geol. Soc., vol. xxii. p. 394 (1866). There, after alluding to the Indian gaur, this author writes, "the term *Aurochs* has been restricted to the European bison by the authority of Buffon, Cuvier, and Prof. Owen; the term *Urox* or *Bos urus*, to the species under consideration [the extinct wild ox of Europe] by Julius Cæsar, Pliny, . . . also by Cuvier, Nilsson, and our great naturalist, Prof. Owen."

Again, in the article on Ruminants by the late Prof. Garrod in "Cassell's Natural History" (2nd ed., p. 35), the term aurochs is applied to the European bison. Finally, we find in Prof. Flower's "Catalogue of Mammalia in the Museum of the College of Surgeons," p. 232 (1884), the animal in question mentioned as the *European Bison* or *Aurochs*.

I find, however, that modern German zoologists (see Brehm's "Thierleben," vol. iii. p. 386) consider it proved that the name *Aurochs* belongs properly to the extinct *Bos primigenius*; and they term the bison, as Prof. Newton states, the *Wisunt*. If this be really correct, English zoologists must accept the emendation. R. Lydekker.

The Lodge, Harpenden, Herts.

The Haunts of the Gorilla.

Concerning Mr. Du Chaillu's saying (see Nature, May 1, p. 19) "that, so far as he is aware, no white man has been able since his time to penetrate to the haunts of the gorilla and bring home specimens killed by himself," I beg to remark that Herr von Koppenfels, in the years 1873–80, killed personally a number of gorillas in the environs of the Ogowé, and sent 3 large specimens, with their skeletons, to the Dresden Museum, some of which I described in the *Mittheilungen aus dem königl. zoologischen Museum zu Dresden*, vol. ii. 1877, p. 230 *seq*. The Museum in Stuttgart also contains several specimens killed by that intrepid traveller; and other museums, I believe—American museums, for instance—possess some. (See also his remarks in the *American Naturalist*, vol. xv. p. 447; and *Die Gartenlaube*, 1877, p. 416 *seq.*, with plate; as well as mine in *Der zoologische Garten*, 1881, vol. xxii. p. 231.) Herr von Koppenfels, who died in the year 1884 in Erfurt, in consequence of diseases acquired in the tropical climate, says (*l.c.*) that the haunts of the gorilla in West Africa are in the forests between the mouths of the Mimi and the Congo Rivers, *i.e.* between 1° N. lat. and 6° S. lat. How far the region extends into the interior is even yet unknown. A. B. Meyer.

Royal Zoological Museum, Dresden, May 7.

Flat-fishes.

Mr. Gulick, in Nature, vol. xli. p. 537, has raised a puzzling point about the flat-fish. In the case of his two Japanese species, it might appear that the ancestor of them both varied in the two directions as to the position of its eyes, &c., and that by the segregation of each form, *neither of which had any advantage over the other*, two species eventually were evolved. But this is not so clear in other cases, apparently. On the American coast of the Pacific, there is a flat-fish, *Paralichthys californicus*, Ayres, which is said by Messrs. Jordan and Goss to be almost as frequently dextral as sinistral. Here, then, is the same sort of variation exactly, yet we see no evidence of segregation and the formation of new species. In the whole subfamily *Soleinæ*, the eyes and colour are on the right side: now, if the "dextral" soles segregated themselves, having no advantage in being dextral rather than sinistral, what has become of all the sinistral ones? If there was no natural selection at play, ought we not to get some sinistral species of *Solea*? Perhaps it may be said that *Solea*, as such, never varied in this way, and was always dextral. But this cannot be so, since we have it on Day's authority that the common sole has a reversed aberration. But, after all, the allied *Cynoglossinæ* are sinistral soles.

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